On Logarithmic Tables.

By J. W. L. Glaisher, B.A., Fellow of Trinity College, Cambridge.

In a paper entitled *Einige Bemerkungen zu Vega's Thesaurus Logarithmorum*, which was published in the *Astronomische Nachrichten*, No. 756, for May 2, 1851 (reprinted *Werke*, t. iii. pp. 257–264), Gauss has examined, at a considerable expenditure of care and trouble, the relative numbers and magnitudes of the last-figure errors that occur in the sine, cosine, and tangent columns of Vega's *Thesaurus* (Leipzig, 1794, fol.). The principle of the investigation is as follows: Gauss remarked that the tabular results in the sine column were almost without exception equal to the sum of the corresponding tabular results in the cosine and tangent columns. Now, if the tabular results in the three columns were all true to the nearest unit, this agreement would only happen on the average three times out of four, so that we may feel certain that one of the columns was obtained by simple addition or subtraction from the other two. It therefore became an object of importance to determine which of the three columns had been deduced (and was therefore liable to greater errors), and to effect this it was necessary to compare some of the (ten-decimal) tabular results in Vega that had elsewhere been calculated to a greater number of places with their more extended values. Gauss accordingly made four series of such comparisons, the scheme of one of which I here reproduce (the comparison being for the 21 angles between 15° 38′ 20″ and 15° 41′ 40″ by means of a fourteen-figure manuscript of Gauss's own calculation):—

	Sine.	Cosine.	Tangent.
0	4	12	1
1	9	8	8
2	6	1	6
3	2		4
4			2

The meaning of this is, that in the 21 angles 4 sines were found correct, 9 affected with an error of 1, 6 with an error of 2, and 2 with an error of 3 in the last figure; and similarly for the cosines and tangents. The scheme I have quoted is a little anomalous, as it gives rather more errors than the average of the whole table would show; but there are objections of a more serious character to the other schemes, that will be referred to further on.

The general tenor of the results of all the comparisons, is that the tangents are the most inaccurate, having much the greatest number of large errors, and that the cosines are more accurate than the sines. This leaves no doubt that the tabular results in the tangent column were obtained by subtracting the

corresponding tabular results in the cosine column from those in the sine column; and the fact that the cosines are more accurate than the sines, Gauss regards as due, at least in part, to the process of interpolation having been simpler for the former, although other causes may have operated. ("Dass die Zahlen der Cosinuscolumne weniger ungenau sind, als die der Sinuscolumne, rührt wohl ohne Zweifel wenigstens theilweise, daher, dass bei den erstern die zur Ausfüllung erforderlichen Interpolations-methoden einfacher ausfallen, möglicherweise können indess noch andere Ursachen mitgewirkt haben, worüber sich nur unsichere Vermuthungen aufstellen lassen würden.")

The explanation which I now proceed to give of the greater accuracy of the cosine column is—there can be little doubt—the true one, as it rests on a statement of Vlacq, the calculator of the table, which Gauss must have overlooked. Before quoting the statement in question, however, it is desirable to explain more fully the origin of the trigonometrical canon that forms rather less than half of Vega's *Thesaurus*, and with which alone we are now concerned. This canon gives logarithmic sines, cosines, tangents, and cotangents for every second from 0° to 2°, and for every ten seconds from 2° to 45° to ten decimal places arranged in the usual semiquadrantal manner. The great bulk of the table was reprinted from Vlacq's *Trigonometria Artificialis* (Gouda, 1633), which gives logarithmic sines, cosines, tangents, and cotangents for every ten seconds of the quadrant; and the values of the functions for every second for the first two degrees were computed by Lieutenant Dorfmund of the Royal Artillery, with the assistance of other members of the corps, under Vega's own direction (*Thesaurus*, p. xxi). What follows must be understood as having reference only to the part of Vega that was reprinted by Vlacq, as I have made no attempt to determine the manner in which Lieutenant Dorfmund performed his portion of the work (amounting to only about a quarter of the whole); Gauss, though noticing the difference of origin of the two portions of the table, has not always kept them distinct in making his comparisons. Now, in the preface to the *Trigonometria Artificialis*, Vlacq himself gives an account of how the canon was constructed, his words being, "Primùm Logarithmos Sinuum à 45 Grad. usque ad finem Quadrantis qui habentur in Opere Palatino, investigavi ope Chiliadum Centum Logarithmorum de quibus supra dixi, juxta modum qui hic traditur pag. 4. quos adeò exactè ita acquisivi, ut in numeris tam magnis supra unitatem in postremâ eorum notâ nihil abundet sive deficiat. Ex illis deinde reliquos omnes Logarithmos Sinuum ab initio Quadrantis usque ad 45 Gradus per solam Additionem et Subductionem inveni juxta hanc Regulam; Duorum arcuum Quadrantem conficientium, si Logarithmus Sinus minoris duplicati addatur Logarithmo Sinus 30 Graduum; et à summâ subtrahatur Logarithmus Sinus majoris, reliquus erit Logarithmus Sinus minoris; Cujus rei demonstrationem videre licet in Trigonometriâ Britannicâ Cap. 16.

Atque sic acquisitis Logarithmis Sinuum, Logarithmos tangentium per solam Subductionem habere potui. . . . Atque ita totum Canonem absolvi."

The formula expressed analytically is

$$\log \sin 2x + \log \sin 30^\circ - \log \sin (90^\circ - x) = \log \sin x$$

so that the process was to calculate first all the log cosines by simply taking out the logarithms of the sines of the angles in the second half of the quadrant as they appear in the *Opus Palatinum*, which gives natural sines, tangents, and secants for every ten seconds of the quadrant to ten places. The log sines were then deduced by means of the formula

$$\log \sin x = \log \cos (90^\circ - 2x) - \log \cos x - \log 2$$

and then the log tangents from

$$\log \tan x = \log \sin x - \log \cos x$$

It thus appears that no interpolation at all (except such as is necessary to take out an ordinary ten-figure logarithm) was used in the construction of the canon; and the reason for the errors being such as they were found to be by Gauss is apparent. The log cosines are merely subject to the ordinary last-figure errors with which the taking out of ten-figure logarithms is always liable to be attended, and which, though frequently as large as ± 1, do not very often amount to so much as ± 2. The log sines, deduced from them by a formula, are much more liable to large errors, as we may expect a large error whenever the log cos $(90^\circ - 2x)$ and log cos x are affected with moderate errors of opposite sign; and, further, as $\log 2 = \cdot 30102\ 99956\ 6398 \ldots$ which would be used as $\cdot 30102\ 99957$, we see that if log cos x is too great by an error α, and log cos $(90^\circ - 2x)$ too small by β, then the error of log sin x is $\alpha + \beta + \cdot 36$.

The log tangents are of course the worst off of all, as in effect they are calculated from the formula—

$$\log \tan x = \log \cos (90^\circ - 2x) - 2 \log \cos x - \log 2$$

so that under the circumstances just mentioned the error would be $2\alpha + \beta + \cdot 36$. Vlacq himself acknowledges his cosines may be erroneous to the extent of a unit (and the error may even amount to 2 units), so that we see that an error of 2 would not be a very unusual occurrence in the sine, nor would one of 3 be so in the tangent, while greater errors still would occasionally arise.

At the end of his *Arithmetica Logarithmica* of 1628, Vlacq gave a ten-figure trigonometrical canon to every minute, the mode of construction of which he explains in the preface to that work in nearly the same words as those above quoted from the preface to the *Trigonometria Artificialis*, with the difference that the original natural cosines were taken from Rheticus's *Thesaurus*

Mathematicus (instead of the *Opus Palatinum*), and that to the statement that the log cosines never differ from the truth by more than a unit, is added the ground Vlacq had for the assertion, "quod ex differentijs differentiarum, quæ ferè æqualiter decrescunt, explorare potui."

The *Thesaurus Mathematicus* contains Rheticus's great canon of sines for every ten seconds to fifteen places, and was published at Frankfort after his death by Pitiscus, in 1613 (misprinted 1513 in the first two title-pages); while the *Opus Palatinum*, which contains, as before remarked, a complete ten-place canon of all the functions for every ten seconds, was calculated by Rheticus, and published after his death, in 1596, at Neustadt, under the editorship of Valentine Otho. A portion of the latter canon, which was found to be partially erroneous, was corrected by Pitiscus, in later copies, on which see De Morgan, Article 'Tables,' *English Cyclopædia*. It thus appears that either the *Thesaurus* or the *Opus Palatinum* would have answered Vlacq's purpose equally well, as (although I have made no comparison to make certain that such is the case) in all probability the last figures of the latter are true to the nearest unit. At all events, Vlacq knew both works, and we may take it for granted that the natural sines which formed the foundation of his table were all but free from error.

I can offer no satisfactory explanation of the fact that Gauss failed to notice Vlacq's own description of the manner in which the canon was constructed. It is possible that he had not a copy of the *Trigonometria Artificialis* at hand to refer to, or that in the copy he did refer to the preface was wanting, as it is hardly conceivable that it should not have occurred to him to see if Vlacq himself had given any information on the matter.

Reverting to Gauss's paper, besides the comparison, the scheme of the result of which was quoted near the commencement of this communication, there are two others which refer to the portion of the table calculated by Vlacq; these are:—

	Sine.	Cosine.	Tangent.
0	51	65	46
1	49	35	43
2			10
3			1

the result* of a comparison for the 100 angles that are multiples of 27′; and formed from the errata-list at the end of Hobert and Ideler's Tables, and

	Sine.	Cosine.	Tangent.
0	29	56	36
1	52	25	42
2			3

* Gauss has included in his scheme multiples of 5′ 24″ also, but I have omitted those that occur only in the tabular results calculated by Dorfmund.

which results from a comparison for every third minute between 14° 0′ and 18° 0′ inclusive, the number of angles being thus 81. This comparison was made by Gauss, with Briggs' *Trigonometria Britannica* (Gouda, 1633), which (besides natural functions) gives log sines and tangents for every *hundredth of a degree* of the quadrant to fourteen places (semi-quadrantally arranged). It thus appears that every third minute is an argument common to both the *Artificialis* and the *Britannica*.

Now a mere glance at the three schemes written above shows that how much more inaccurate are the sines and cosines in the first than in the other two, where they appear as quite free from errors greater than unity. This, Gauss himself remarked, and he pointed out that errors greater than unity were not unusual for arguments that do not occur in the *Britannica*. This, at first sight, might seem to imply that Vlacq had made use of the *Britannica* (which he printed at his own expense after Briggs's death) to correct his own canon by. It is a *primâ facie* argument against this view, that if Vlacq had corrected any of his values, he would have corrected them thoroughly, and not consistently left a unit error remaining; but there is no occasion to appeal to probable reasoning at all, as the perfect independence of Vlacq's and Briggs's calculation is placed beyond all doubt by the fact that all the tabular results dependent on arguments common to both the *Artificialis* and the *Britannica* had appeared previously in Vlacq's *Arithmetica* of 1628. This is evident, if it is remembered that the latter work contains a *minute* canon, and that the arguments common to the two former works are all multiples of 3′. The tabular results in the *Arithmetica* were reprinted without alteration (at all events generally) in the *Artificialis*, as I have found by comparing them for every third minute between 14° and 18° (the angles included in the last-written scheme), and by miscellaneous comparisons of different pages of the two works chosen at random.

The difficulty of seeing any explanation that would account for the greater deviations from accuracy in the case where the arguments are not included in the *Britannica*, induced me to form independent comparisons between the *Britannica* and the *Artificialis*, and also between the *Britannica* and Vega's *Thesaurus*, for the same arguments as those to which Gauss's schemes apply. The results were as follows:—

A comparison of the tabular results for the 100 angles that are multiples of 27′ between the *Britannica* and *Artificialis* gave the following scheme:—

	Sine.	Cosine.	Tangent.
0	43	64	41
1	49	36	41
2	7		15
3	1		3

and the comparison between the *Britannica* and Vega's *Thesaurus*, for the same arguments, gave

	Sine.	Cosine.	Tangent.
0	51	64	46
1	49	36	43
2			10
3			1

The results of a comparison between the *Britannica* and *Artificialis*, for the tabular results corresponding to every third minute between 14° and 18° (81 angles), are

	Sine.	Cosine.	Tangent.
0	25	57	34
1	52	24	41
2	4		6

while the comparison between the *Britannica* and Vega's *Thesaurus*, for the same arguments, gave

	Sine.	Cosine.	Tangent.
0	29	57	36
1	52	24	41
2			4

The first of these four schemes was verified also by comparing the *Artificialis* with the table in Callet for every thousandth of a degree (which was, however, no doubt formed from the *Britannica*). The second agrees with the results found by Hobert and Ideler, and quoted by Gauss, except that the number of errors in the cosine-column is given in the latter as 35 instead of 36. This arises from Hobert and Ideler not having included the error in 45°, thinking perhaps it was sufficient to notice its occurrence in sin 45°; the other two schemes call for no particular remark, except that it will be noticed that the last differs slightly from that given by Gauss, who seems to have made one or two trifling mistakes.

A glance at the two Vlacq schemes shows how exactly the distribution of the errors in the different columns is what we would expect from our knowledge of the manner in which they were calculated.

The nature of the corrections made by Vega is also rendered very apparent, viz. he corrected nearly all the errors amounting to as much as 2 in the sine-column,* the additional accuracy of the tangents being consequent thereon. How Vega found out these errors, I do not know: it could not have been by comparison with

* In the first of the two comparisons, I marked each tabular result with a 0, 1, 2, 3, according as it was correct or was in error, by one, two, or three units, and I find that every error amounting to 2 or 3 was corrected, but not a single one merely requiring an alteration of one unit was amended.

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the *Britannica*, or he would have probably referred to it; but there is nothing in the *Thesaurus* to show that he even knew of its existence, much less made any use of it. Besides, considering Vega's love of accuracy (not to mention the fact of his having offered a reward for the detection of errors in his work), it is impossible to believe that had he used the *Britannica*, he would have been contented to leave unaltered so many unit-errors that he might have corrected, and only to remove those of greater amounts; while he certainly would not have allowed the errors of 2 in the tangent-columns to remain. Also, it will be observed that Gauss's comparison for angles not an exact number of minutes (see first scheme quoted in this paper), seems to show that such corrections have not been generally made in cases where the argument involves seconds as well as minutes. A theory that might explain this would be to suppose that Vega had corrected Vlacq's minute canon of 1628 by second differences; but I refrain from making conjectures, as a more detailed examination of the table than I have given to it could not fail to reveal the method that had been adopted. Such an examination I hope to be enabled to make, as it appears to be of very considerable practical importance to know the exact extent to which Vlacq and Vega (both of which, be it remembered, so far from being obsolete and only historically valuable, are unique of their kind, and still in daily use) are to be relied on. It is rather a scandal to the mathematical sciences that so little should have been done to improve upon the calculations of two centuries and a half ago. It is the knowledge of the extreme value of these fundamental ten-figure tables in mathematical calculations of all kinds, together with the conviction that the time must come when their republication will be a necessity, that appears to me to render the question of their accuracy one of the very highest importance; quite independently of the historic interest that every one must feel in the origin (or rather birth) of the quantities which are the means whereby every important numerical calculation is performed, and the instrument by which theoretical is transformed into practical science; and I may here remark that there is scarcely one of the principal mathematical tables that was not originally professedly calculated chiefly for its use in Astronomy (by facilitating the solution of spherical triangles).

I had the curiosity to make a comparison between the *Britannica* and *Artificialis* for arguments near the middle of the quadrant (where the sines and consines are nearly equal). The following shows the results for the 61 angles, which are multiples of 3′ between 43° and 45°:—

	Sine.	Cosine.	Tangent.
0	36	43	36
1	23	18	17
2	2		8

and it will be seen that the relative accuracy of the sine, cosine, and tangent columns remain the same in all essential respects.

Gauss, in his paper, remarked as one of the results of his examination, that for the angles $15^\circ\ 40'\ 20''$ and $15^\circ\ 41'\ 30''$ the errors for the log sine, cosine, and tangent are respectively $3, -1, +4$ and $+3, 0$, and $+4$. I have examined these by substitution in the formula quoted above, from which Vlacq computed his log sines, and I find that it is rigorously satisfied. This would be a verification (if such were needed) that Vlacq did calculate his log sines in the manner he explains, and it further indicates that the log sine of $31^\circ\ 20'\ 40''$, and that of $31^\circ\ 23'$, are inaccurate by at least a unit.

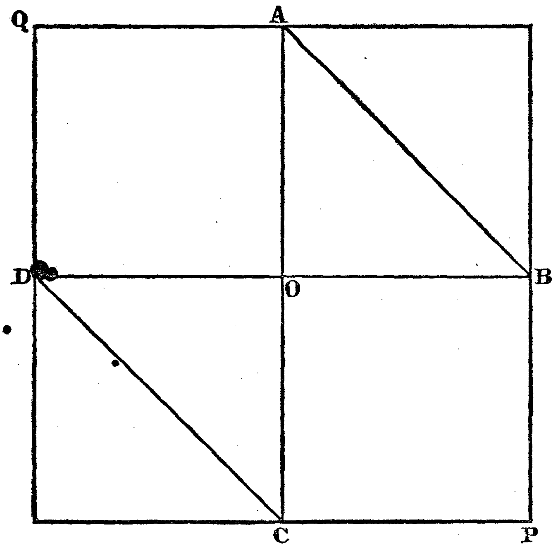
It appears, therefore, from the considerations developed in this paper that there is no mystery about the errors in Vlacq's *Trigonometria Artificialis*, their origin and magnitudes being accounted for in a perfectly satisfactory manner. But what is still left uncertain is the manner in which Vega examined and corrected the table; it seems that his method enabled him to remove large errors from the sine columns* (but not from the tangent columns), at all events for certain arguments that were an exact number of minutes, but not apparently for all the rest.

In the preface to his *Thesaurus* Vega offers a reward of a ducat for the first notice of any error which could give rise to a false calculation;—(German, "Der zu falschen Rechnungen Anlass geben kann;" Latin, "Pro singulis spalmatibus computationem turbantibus.") Whatever may have been the exact meaning Vega ascribed to these words (and, be it noted, he points out in the same preface errors of a unit in the last figure that he had detected in Vlacq), it is clear that had they been taken literally, he might have been awkwardly placed, as Gauss estimates the number of last-figure errors at from 31983 to 47746, and in fact, as the latter has remarked, been in much the same position as King Shiram. It will have been observed that Vlacq cannot fairly be charged with more than a few last-figure *errors*. He is responsible for the cases where in the log cosines the last figure deviates from the truth by more than a unit, and these may properly (with the accompanying deviations in the log sines and tangents) be laid to his charge as blunders made by him; but having stated that all his log cosines may be inaccurate to the extent of ± 1, and having explained his mode of calculation of the log sines and tangents, he throws upon the reader the responsibility of determining how far the latter are to be relied on.

The statement quoted from Gauss to the effect that an irrational quantity given to a fixed number of decimals, and obtained as the sum of two other irrational quantities similarly curtailed,

* Nearly all the errors which Vega mentions in his preface as having been corrected by him refer to arguments containing some seconds over; this would seem to imply that he set more value on the detection of such errors, than those having reference to arguments consisting of an exact number of minutes.

will be erroneous on the average once out of every four times, amounts to the assertion that if x and y can each have any values between $\pm a$, and all these values are equally probable, then the chance that $x+y$ lies between $\pm a$ is $\frac{3}{4}$, or in other words that $\iint dx\,dy$ subject to the conditions $a > x > -a$, $a > y > -a$ and $a > x+y > -a$ is equal to $\frac{3}{4}a^2$. This can of course be proved by performing the integrations, but its truth is evident at sight by inspection of the subjoined figure, where A B P C D Q is obviously the area included within the limits of integration.



If $x-y$ were involved instead of $x+y$, AD and BC would be joined instead of AB and DC, and the result would be (as it clearly ought to be) the same as before.

Michael Taylor's trigonometrical canon to every second was calculated throughout to ten figures, and then contracted to seven, so that the results in this case ought to agree with the theory. I accordingly opened Shortrede (reprinted from Taylor) at random, and had the cases that occurred in the next few pages, where the log sine was not the sum of the log cosine and log tangent, noted down.* The result was that the numbers of "disagreements" in each minute for the 20′ between 21° 15′ and 21° 34′ (inclusive) were 15, 13, 16, 11, 19, 20, 13, 13, 27, 14, 15, 13, 18, 18, 15, 13, 14, 7, 11, 15; thus there are 300 disagreements out of 1200 cases, or the ratio is exactly $\frac{1}{4}$ (the *exactness* being merely a coincidence, of course; it is curious that Gauss met with a similar coincidence in a trial of the same kind that he made with 900 arguments). It would be worth while (and I hope to be able to have it done at some future time) to form a similar ex-

* In making an examination of this kind care must be taken to omit the bottom line of each page (or to allow for it if included), as it is repeated on the next page: $x^\circ\ 60' = (x+1)^\circ$.

amination for the whole of the canon as it appears in Shortrede, not with a view of verifying the result given by the theory of probabilities, but to examine the different results that might appear to follow by taking separately different portions of the table, &c. Of course the theory of probability *must* be right, and if a series of facts do not agree with it, it is merely so much the worse for the latter; or in more accurate language, it merely shows that the practical case is not of the kind contemplated in the theory, so that in strictness the latter can never be *verified.* But what is of interest in a series of facts to which the theory does apply, is to watch the manner in which the absolute deviations from the law may increase, while the relative deviations decrease. One usually has recourse to tosses of coins, throws of dice, &c., for such illustrations, but the comparison just suggested would present the great advantage of enabling any one to examine the "runs of luck," &c., which would be always open to verification, while in the former cases they must be received on the *ipse vidi* of the experimenter who might have been mistaken.

In relation to a subject on which I commented at some length in my last communication to the Society on logarithmic tables (printed in the *Monthly Notices* for March, 1873), I wish to call attention to a remarkable paper by A. Gernerth, entitled 'Bemerkungen über ältere und neuere mathematische Tafeln,' Vienna, 1863 (reprinted from the *Zeitschrift f. d. österr Gymn. Heft* vi. p. 407), from the contents of which the views I then expressed on the necessity for some body exercising a continual revision over tables receives a most remarkable confirmation. My attention was called to this tract by Professor Bierens de Haan, in consequence of a paper of mine in the *Philosophical Magazine* on a cognate subject, and although a copy of it was lying on the table before me at the time I was correcting the proof-sheets of my last paper, I had not had the opportunity to do more than glance at its contents, else I would have added a note containing the remarks I now place here. The author has examined a great number of tables (by eighteen authors), with the view of testing their accuracy, but he has confined himself to the determination of the correctness of the last figure only. This he has done advisedly, rightly considering that the attention paid to secure accuracy on this point properly represents the care and competency of the editor; although his results would have been of more practical value if he had examined all the figures, as in point of fact no one relies on a result given by means of logarithmic tables as really accurate, it being well known that errors of even two or three units in the last place may occur, so that a last-figure error of unity is *practically* to the user of a table of not much importance. Still, as Herr Gernerth has remarked, the last figure must be printed, and so it had better be printed correctly, at all events, when an ordinary amount of care on the editor's part would ensure such being the case. Any one, however, who will look at Herr Gernerth's paper will be astonished at the extraordinary

amount of carelessness shown in this respect; for instance, in Gronwaldt's six-figure chord table (Quedlinburg, 1850), in 1920 tabular results, there appear 648 such errors; in Rühlmann's six-figure logarithmic and trigonometrical tables (Leipzig, 1859) 1493 errors were found in 31680 tabular results, and so on; all of which ordinary care would have avoided. In Domke's nautical, &c., tables (six figures) 2·58 of the tabular results examined were found to be inaccurate in the last figure; Hülsse's Vega (1849) has 5·56 per cent thus erroneous. And these are far from being the worst (Beskiba's *Lehrbuch für . . . Arithmetik* occupies this position, having 70·28 per cent of the results incorrect, but this is seen to be due to a constant error.) The tract in no way occupies the ground traversed by my paper, being in fact complementary to it; for while I was concerned merely with a certain class of hereditary errors occurring in the logarithms of numbers, Herr Gernerth considers only such as are due to very different causes, and most of his results have reference to the logarithmic trigonometrical canon. The conclusion, however, that he draws relative to the exceedingly unsatisfactory way in which mathematical tables are edited affords a striking confirmation of what I found; and while my investigation showed rather the incompetence and want of knowledge of the average run of editors, he points out forcibly their carelessness; and, what is worse (with the exception of Schrön's beautiful tables and a few others), there is very slight tendency exhibited in the direction of a higher degree of accuracy. With regard to the per-centage table on p. 39 of Herr Gernerth's tract, it is especially to be noticed that they only have reference to the errors found by himself, and not to the total number known to exist: thus, if his examination gave 100 errors, of which 50 were known previously, he would only in forming the per-centage for that table have treated it as containing 50 errors. This mode of formation deprives the list of much of the interest which otherwise it would have possessed in a high degree, as the point with which mathematicians and users of tables are concerned is the number of errors that any particular work contains, not merely the number for the discovery of which we are indebted to Herr Gernerth. In the tract the author adverts to the care taken in correcting the succeeding editions of Callet, which seems to have been very much less than I without examination had imagined (*Monthly Notices*, March, 1873). It is proper also to state that he gives a long list of 598 errors that he has detected in the *Opus Palatinum*, and of 88 errors additional to those given by Vega, in the *Thesaurus Mathematicus* of 1613.

With regard to what constitutes an error, I cannot quite agree to Gauss's definition that any deviation, however slight, from the highest attainable accuracy should be called an error, and the author reprobated accordingly. In the *Monthly Notices* for May, 1872, I remarked that, supposing the seventh, eighth, ninth, and tenth figures of a number were, say 6499, it seemed to me that the seventh figure contracted was with equal accuracy,

as far as the user of the table was concerned, either 6 or 7, and that no computer should be expected in a case of this kind to continue his calculation to more places for the purpose of making a decision according to an ideal standard of accuracy, nor need we feel any gratitude to any one who did take such trouble. In fact, the proper standard ought to be, I think, that the tabular results should never be erroneous by more than ·555... of a unit in the last figure, and that this should be the criterion adopted. However, in contracting a ten-figure table to seven figures, or in any other case, the nearest approach to the truth should invariably be given. The last figure, as before remarked, must be printed, and so it may as well be as near the truth as possible, viz. an editor should not be required to take any trouble to reduce the error from ·555... to ·500...; but, if it is no more trouble to him to so reduce it than not to do so, the more accurate value should be given. In fact, there is *some* reason for the greater accuracy, but it is so small that almost any argument on the other side drawn from convenience would outweigh it. As might be gathered from my remarks about Vlacq, it seems to me that an author who explains his method fully, and the steps he took to ensure accuracy, accomplishes his part of the work, the errors that arise (except such as are due to carelessness) being merely instances of want of completeness (which a succeeding computer may perfect), and not errors in the sense of conveying reproach. I ought also to mention on the subject of logarithmic tables Professor Bierens de Haan's *Iets over Logarithmentafels*, Amsterdam, 1862, 8vo. (reprinted from the *Mededeelingen der Koninklijke Akademie van Wetenschappen*, *Afdeeling Natuurkunde*, Deel. xiv), which contains an extensive bibliography of the subject.

I here append a letter relating to my two communications in the May and June numbers of the *Monthly Notices* of last year, which I had much pleasure in receiving from Mr. J. N. Lewis, of Mount Vernon, Ohio.

"Dear Sir,—I have lately received Vol. xxxii. of the Royal Astronomical Society's *Monthly Notices*, and have read with much interest your articles (pp. 255 and 288) 'On Errors in Vlacq's Tables of Ten-figure Logarithms of Numbers.' Having myself collected together some of the most noted tables of logarithms—a few of them old ones—I occasionally spend a leisure hour in looking over them, and making here and there a note. I have a copy of the 'Miller' edition of Vlacq, and just before receiving the volume of the *Monthly Notices*, containing your articles, had copied from it and sent to the officers of our Coast Survey a small list of corrections not contained in the list of errata by Vlacq himself, nor in those of Sherwin and Lefort. I found by the reply, however, that our Coast Survey Office does not possess a copy of Vlacq. In the Congressional Library there is a copy of the *Trigonometria Artificialis*, but none of the *Arithmetica Logarithmica*. And here I would remark that your

note (bottom of p. 257) that 'this list' (of errata) 'does not occur in Miller's copies,' is not entirely correct. My copy is one of Miller's, 'Printed *by* George Miller,' not '*for* George Miller,' as your article reads, p. 256, line 2; and it contains the errata table, headed 'Faults escaped amend thus.' Sherwin says, 'Vlacq's own Errata Table is found in few of his books,' but he makes no distinction between the Latin and English copies.* Indeed, he makes no allusion whatever to the English copies. It would seem, then, that the errata table was wanting in some of the Latin copies, as well as the English. The number of printed errata in my copy is 120 (in 3 columns), as stated by you. This copy contains the autograph of Michael Taylor, and, as is clear from manuscript notes, has been used by him, or some other person, in preparing 7-figure tables. The corrections (with a pen) seem to be in an older hand than Taylor's, and may possibly have been made by Gardiner, who says (preface to his Tables) 'I likewise examined and corrected Vlacq's 100 chiliads of logarithms, and their differences, with the utmost attention.' However this may be, the corrections seem to have been made with the greatest care. Something like a year ago I compared them with M. Lefort's table (*An. de l'Obser. Imp. de Paris*, t. iv. pp. [148] et seq.), and found that *every* important error noted by Lefort had been discovered and corrected, and a great part of the unimportant ones also—unity in the 10th decimal place—so that my copy of Vlacq has been much more carefully corrected (by Taylor, or Gardiner, or some other former owner) than Maskelyne's, in which you say, p. 259, that 265 of Lefort's errors are not corrected, 43 of which are serious. I find, too, that *all* those in your table (*Monthly Notices*, xxxii. p. 258) have been discovered and corrected in this old volume. Even the logs of 11275 and 54040 are corrected. I further find, Diff. to log of 9552, for 45639, read 54639. This correction is not in your table, nor any other that I have seen. The log of 26613 has the 8th decimal (3) made with a pen. The same is the case with the 8th decimal (6) of the log of 33509, the 7th decimal (6) of the log of 40217, the 6th decimal (3) of the log of 56359, and the 3rd decimal (9) of the log of 69163; but whether these were defectively printed figures, afterwards amended with a pen, or not, I cannot tell. The log of 57756, noted as erroneous by Sherwin, is correct in my copy of Vlacq. I have examined the 25 numbers given by you at the bottom of p. 260 as imperfectly printed, and find the following to be the case in my copy:—576 is nearly perfect; 4576, the same; 7106, the 6 somewhat imperfect—no danger of mistaking it; 16826, nearly perfect; 19650, the 9 rather imperfect; 21286, perfect; 24077, the 4 rather imperfect—no danger of mistake; 30420, the last 0 a little imperfect; 31176, the 6 rather imperfect; 31226, the 6 imperfect (dim); 33326, the 6

* Some copies of the *Arithmetica* of Vlacq had the explanations in French. See *Advertisement* to Callet's Tables; Thomson's *History of the Royal Society* p. 262, &c.

somewhat imperfect; 37088, nearly perfect; 41426, perfect; 45876, perfect; 61226, the last 6 imperfect (dim); 61526, the last 6 imperfect; 66876, perfect; 66896, the last 6 imperfect; 81026, the last 6 imperfect; 83864, the 6 nearly perfect; 96903, the 0 imperfect; 97318, the 8 is dim (also the last figures of 97319 and 97320); 97326, nearly perfect; 97328, the 8 rather dim—no danger of mistake; 98280, the 0 imperfect.*

"In noticing M. Lefort's correction to the log of 53053, you say 'the correction probably has reference to some other number.' I found, some time ago, that this correction most probably had reference to the log of 53050. M. Lefort's errata table requires also the following corrections, not noted by you:—At Number 8155, in col. of errors, for 3·22, read 5·22; and at N. 39626, for · read 2. (I think the · in M. Lefort's copy of Vlacq must have been caused by a defect in printing, for mine has 2, as it should have.) M. Lefort notes with an asterisk such errata as are common to Vlacq and to the *Thesaurus* of Vega, but he nowhere informs us whether there may not be errors in Vega which are not to be found in Vlacq. In fact, he does not appear to have examined the *Thesaurus* any farther than to compare it with the errata found in Vlacq. I think this is to be regretted, for Vega's book is considerably used at the present day, whereas Vlacq's is no longer procurable. You say, however (p. 262), that Vega's work is scarcer in England than Vlacq's; but I found no difficulty in procuring a copy (from Germany). It seems to me, therefore, that M. Lefort, or M. Houel, or Dr. Bremiker, or some other patient editor, would be doing the mathematical world a service by making a searching and critical examination of Vega's *Thesaurus*, both as to the Numbers and the Trigonometrical Canon. In the latter, Dr. Bremiker says the uncertainty in the last figure amounts to 4 units. (For a few corrections of this Canon see the tables of Hobert and Ideler, p. 350).

"At p. 260 you give a list of errors in the column of 'Num.' in Vlacq's *Arithmetica*, which have not heretofore been noticed. To this list I would add the Num. 347, and here I would say that I cannot be sure that my copy of Vlacq (or Miller) does not contain other manuscript corrections not heretofore noticed by the editors of later tables, nor contained in any of the tables of errata you have examined, for I have only given the matter a few hours' examination, for the purpose of sending a few corrections to the officers of our Coast Survey, as stated above.

"You say, p. 257, 'In 1631 Vlacq published his *Trigonometria Artificialis*.' I have not a copy of this book, but I have never elsewhere seen the date of publication other than 1633. Allow me to correct two or three other small oversights:—p. 261, lines 7 and 6 from bottom, for 'seventh, eighth, and ninth,' read

* [The letter contains sketches showing the shape of most of the figures noted as imperfect.]

'eighth, ninth, and tenth;' p. 262, middle of page, for 'Adrian Vlacq,' read 'Adriani Vlacci;' and for '305 pp.' read '307 pp.' (The logs occupy pp. 3 to 309, both inclusive;*) p. 288 I find the following: 'It does not follow that the number of errors found by Vega in 452—301=151,' &c. Should not this read, 'It does not follow that the number of errors *not* found *in* Vega *is* 452—301=151?' &c. On the same page, bottom, the correction of log of 64818 is given. This had been noticed by you before, p. 258. I may note here on the matter you refer to on p. 289, that my copy of the *Thesaurus* of Vega has only *one* page of errata, p. xxx. of the Introduction, the same errata being repeated, however, in a little different form on p. 685.)

"I see you refer (note, p. 259) to the question of the date of the 2nd edition of Sherwin's *Mathematical Tables*. I have four editions of Sherwin, the title-pages of which bear the dates 1717, 1742, 1761, and 1771. The last three are called the 3rd, 4th, and 5th editions, and that of 1717 I consider the 1st edition, with a new title-page added, a thing not uncommon, I believe, in those days (nor at the present time, either), when part of an edition remained unsold for some years. There are some reasons which induce me to think that nothing but the title-page has been changed, among which are—1st, there is nothing said anywhere of its being a second edition; 2nd, there was certainly an edition (the 2nd, I believe) issued in 1726; 3rd, this 1st edition (as I call it) contains Sharp's logarithms to 61 decimals for all numbers under 100 and of all primes under 200 (some of them, however, short of 61 by 2 to 10 decimal places). Now, Sharp's *Geometry Improved* appeared the same year, 1717, of the date of the title of this edition of Sherwin, and it (the *Geometry Improved*) contains the logarithms of all numbers to 100, and of all primes under 1100 to 61 decimals. If, then, this edition of Sherwin was really of the date 1717, why should it not contain these additional logarithms of Sharp, as subsequent editions of Sherwin do? Again, Gardiner, who edited the 3rd edition, so called, certainly knew how many editions had gone before; and further, the copy I have, with date 1717 in the title, contains a second title-page, at the beginning of the tables: 'Mathematical Tables, London: printed by *S. Bridge*, for *Jer. Seller* and *Cha. Price*, at Hermitage Stairs, in Wapping; and *John Senex*, next Door to the *Fleece Tavern*, in Cornhill, 1705.' This last date is, I have no doubt, that of the 1st edition, though Hutton, and others following him, make it 1706, for what reason I do not know. All agree that the dedication is dated July 12, 1705. Dr. Bremiker gives 1705 as the date of the 1st edition, and so does Lalande, Astron. (3rd edition) § 4104; but in his *Bibliographie Astronomique*, p. 366, gives 1706, when also he gives the dates of other editions, 1717, 1726, 1741, 1742, 1761, and

* Same place, for 100,100, read 101,000.

1771. I know nothing of an edition in 1741, though Callet gives that date also, as well as 1724. Hobert and Ideler (see *Neue Trigonometrische Tafeln, Einleitung*, p. xliii) give 1741 as the date of the 3rd edition, and it seems certain there were copies bearing this date. See also Thomson's *History of the Royal Society*, p. 262.* Barlow, *Mathematical Dictionary*, gives both 1704 and 1706, *in the same paragraph*, as dates of the 1st edition; but there are so many errors in the article 'Logarithms' in this Dictionary, that it is useless to quote from it. Delambre (*History of Modern Astronomy*, vol. ii. p. 90) says he possessed the editions of 1717 and 1726. He gives the date of the 1st edition, 1706, only as a conjecture of Lalande.

"What little examination I have given to logarithmic tables has been mainly induced by reading Professor De Morgan's very full and interesting catalogues of such tables in the *Penny Cyclopædia* and the *English Cyclopædia*. I have found Professor De Morgan's descriptions of tables to be, in general, quite correct—so far as I have been able to verify them—yet, at the same time, he has committed a few oversights. I will give only one example. He says Delambre is wrong in saying that the tables of Hobert and Ideler (Berlin, 1799) subdivide the quadrant as minutely as those which himself and Borda published; which latter he (De Morgan) describes as follows:—every $1''$ from 0 to 3°, every $10''$ from 3° to 40°, every $1'$ from 40° to 50°. Now, if dividing the quadrant to a certain degree of minuteness means that, to that degree of minuteness, the logs of the functions can be read out of the tables immediately, and without adding proportional parts, then the above description is wrong, though it is called a correction of a former description given in the *Penny Cyclopædia*, article 'Tables.' The correct description is as follows:—

"To every $10''$ from 0 to 3°, with proportional parts of differences for each single second.

"To every $1'$ from 3° to 50°, with proportional parts of differences for each $10''$.

"There is absolutely no difference between the 40° to 50° part of the table, and the 3° to 40° part, except that in the former the proportional parts are arranged differently, and given for each $5'$ only, so as to economise room. Delambre's description is correct. See his *History of Modern Astronomy*, vol. i. p. 556; also same vol., p. 557, and his preface to Borda's Tables, p. 113, where he says Hobert and Ideler's Tables are of the same extent† as Borda's. In this matter—of the minuteness with which Borda's Tables subdivide the quadrant, as compared with Hobert and Ideler's—even the careful Dr. Bremiker (see his preface to

* Where, with the date 1706, the title of *Gardiner's* tables is given, not *Sherwin's*.

† Hobert and Ideler's Tables have no proportional parts, otherwise they (in the trigonometrical parts) are the same as Borda's.

Vega's 7-figure logarithm, p. iv.) has fallen into the same error as Prof. de Morgan; perhaps has copied from the latter.

"I have some other notes on logarithmic tables, but the above must suffice for the present.

J. N. LEWIS.

Mount Vernon, Ohio, U.S.A.
March 12, 1873."

In reference to the contents of this letter I have to remark that copies of the (Miller) Vlacq of 1631 having an errata list must be very rare. I have carefully examined four Miller copies, and none of them contain any such list, or any reference to one; and as remarked in my last communication to the Society on the subject of logarithmic tables, it is clear that John Newton in 1658 reprinted from a copy without an errata list. Vlacq, in his work of 1628, not only gives the list of errata, but refers to it in his preface, so that I imagine all the Latin copies have the list, but in none of the Miller copies I have seen does Vlacq's preface appear. The existence of the French edition I alluded to in a postscript to my last paper (and previously in the *Phil. Mag.*), and I there also noticed the confirmation of my opinion that there was also a Dutch edition that was afforded by a statement I quoted from a letter of Briggs. I have since written to Professor Bierens de Haan at Leyden, and asked him to see if there exists any such copy in any of the libraries there, but he informs me that he can find no trace of any edition having appeared with the introduction in Dutch. I still, however, think it likely that the tabular portions of the Miller copies were originally intended to form part of a Dutch edition.

The knowledge of the whereabouts of Michael Taylor's copy of Vlacq is interesting, and it is valuable to know that all the important errors found by means of the French manuscript tables were known to Taylor. In my last paper I showed that every error that can affect a seven-figure table of the logarithms of numbers had been published by 1794, and Mr. Lewis finds that every important error (*i. e.* every error greater than a unit in the last figure) had been discovered before that date, though of course it does not follow that they had all been published, for I believe that neither Gardiner nor Taylor gave publicity to nearly all the errors they discovered. The error in the Diff. to log 9552 is pointed out by Vlacq in his own errata list. With regard to the alterations which Mr. Lewis considers to have been made by the pen, I find by examining the logarithms in question in three copies (one of 1628 and two of 1631) now open before me, that the logs of 26613, 33509, and 69163 are correctly and clearly printed in all three; that the top of the 6 in log of 40217 is out of its place in all three, the type having been damaged; that in the log of 26613 the 3 is exceedingly faint in the two Miller copies, in which also the lower part of the 3 in the log of 56359 is blotchy; none have been touched with the pen, and the faint 3

in the log of 26613 is the only one requiring improvement. From the Latin copy being the best it may be inferred that the tables for the 1628 edition were printed off before those that were subsequently used in the Miller copies.

With regard to M. Lefort's erratum in log of 39626, I have the 2 in all stages, in one copy (1628) it is printed quite clearly, in another with the upper part illegible, and in the third it appears only as a point. All the oversights that Mr. Lewis kindly points out as occurring in my paper are just, including "*by* George Miller" for "*for* George Miller." The date of the *Artificialis* is 1633.

The "number of errors found by Vega in 452—301=151" conveys a meaning exactly opposite to what was intended through a misprint. I wrote *is*, not *in*, and when this correction is made, the remark is to the effect that although Vlacq contains 452 errors and Vega 301, it does not follow that Vlacq detected and corrected 151 of the former (as the two tables are not of equal extent), so that my meaning was the same as that suggested by Mr. Lewis. I entered into so much detail about Sherwin's tables in my last paper that it is scarcely necessary to add anything further here. It will be seen that all the editions noticed by Mr. Lewis had come under my own eye. The existence of copies with the date of 1706 is certain. One is before me now, and I think I have seen two copies with this date, and two with that of 1705, so that as far as this is any guide, the numbers of each may be about equal. Though I do not think the argument derived from the logarithms in Sharp's *Geometry Improv'd* is of much weight, still it seems most probable that the first impression formed the copies of 1705, 1706, and 1717.

Having had occasion to examine a very great number of the works noticed by De Morgan in his article in the *English Cyclopædia*, I can most heartily endorse Mr. Lewis's opinion of the accuracy of the descriptions contained therein; and only those who have tried know the extreme amount of care that is required in order to describe truly the contents of a number of tables, with their different arrangements. In De Morgan's article the notices of the tables are too brief to be very useful, but their closeness to the truth is admirable. Curiously enough, in describing Borda and Delambre for the report of the British Association Tables Committee I fell into the same mistake that De Morgan did (and as I now learn for the first time from Mr. Lewis that Bremiker did also), and only discovered the error in a subsequent revision, when I noticed a trifling inconsistency, to correct which rendered it necessary to look at the work itself again. The circumstance afforded me some uneasiness at the time, when I considered that but for an accident it might have appeared that the general statement prefixed to the Report to the effect that the descriptions had been made solely from inspection of the works themselves was subject to exception. It must be confessed that the part of the table beyond 10′ is a trap in which any one used to

describing tables would be most likely to be caught, as the arrangement is so exactly similar to what it would usually be, if the contents were as described by De Morgan, that to all who had not absolutely used it such would seem to be the case. It is not easy at first sight to see why the log sines and tangents were not given to seconds, &c., instead of the room they would have occupied being devoted to the proportional parts; but the explanation of course is that by the actual arrangement the log sines and cosecants, tangents and cotangents, &c., are placed on an equality, as the proportional parts are the same for both. It does not seem very clear that the choice was a wise one.

I will conclude with the remark that the fact of my former papers having elicited the above interesting letter from Mr. Lewis, affords a justification for my having made these communications to the Society in an avowedly imperfect state, without delaying their publication till I was satisfied that they were as complete as I could make them. It is only by the co-operation of several workers, possessing different sources of information, that accuracy or high excellence in this direction is attainable.

Cambridge, April 11th, 1873.

Postscript.—Mr. E. A. Hadley has pointed out to me that the statement on p. 337 of my paper *On the Progress to Accuracy of Logarithmic Tables* in the March *Notice*, that 'the two errors which, though discovered by 1794, exhibit such a persistent vitality, are those in log 38962 and log 52943, and [that] they occur in all the tables subsequent to 1827, where two or more are assigned in the list,' is not quite accurate. In fact, log 38962 is correctly given in Hassler, 1830, the tables published in Chambers' Educational Course, 1847 and 1853, and Callet, 1855. It is however quite true that the errors in the two logarithms in question are the most persistent, in the sense that they have survived the longest, as they both appear in Sang, 1871. On referring to the original comparisons whence the list on p. 335 was compiled, I find that of the thirty-one works (leaving out Vlacq itself) included therein, log 52943 is inaccurate in 25; log 57628, log 57629, and log 63747, in 17; log 38962 and log 67951 in 16; log 53919 in 15; log 24626 in 14; log 33071 in 13; log 81674 in 12; log 60844 in 9; &c. Thus log 52943 is *facile princeps*, and the only tables (leaving out of considerations the four that are quite free from error) in which it does not occur are the Vegas of 1794 and 1797.

May 21st, 1873.